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ABSTRACT

The most widely used procedures to harness the power of a concomitant (nuisance) variable are block designs and analysis of covariance (ANCOVA). This study attempted to provide a scientific foundation on which to base decisions on whether to block or covary and how many blocks to be used if blocking is selected. Monte Carlo generated data were analyzed using one-way analysis of variance (ANOVA); two-block, four-block, and eight-block designs; and ANCOVA. Resulting empirical powers were entered into a repeated measures four-way factorial design with three factors representing different experimental conditions and one factor representing the five procedures being compared. The results indicated that the correlation coefficient between the concomitant and dependent variables was the critical factor to influence the choice. One-way ANOVA was the best choice when there was no relationship, blocking was preferred when the correlation was low, and ANCOVA achieved the highest power when the correlation was high. Block designs and ANCOVA became more powerful and the optimal number of blocks increased as the correlation coefficient, the number of treatments, and the number of subjects per treatment increased. Five appendixes provide four tables of supplemental information and computer codes used in the analysis. (Contains 5 tables and 44 references.) (Author/SLD)

TO BLOCK OR COVARY A CONCOMITANT VARIABLE:

WHICH IS MORE POWERFUL?

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"TO BLOCK OR COVARY A CONCOMITANT VARIABLE: WHICH IS MORE POWERFUL?"

The most widely used procedures to harness the power of a concomitant variable are block designs and ANCOVA. The decisions on whether to block or covary and how many blocks to be used if blocking is selected are often based on rules of thumb with little empirical support. The purpose of this study is to provide a scientific foundation on which to base such decisions.

Monte Carlo generated data were analyzed using one-way ANOVA; two-block, four-block, and eight-block block designs; and ANCOVA. Resulting empirical powers were entered into a repeated measures four-way factorial design with three factors representing different experimental conditions and one factor representing the five procedures being compared.

The results indicated that the correlation coefficient between the concomitant and dependent variables was the critical factor to influence the choice. One-way ANOVA was the best choice when there was no relationship, blocking was preferred when the correlation was low, and ANCOVA achieved the highest power when the correlation was high. Block designs and ANCOVA became more powerful and the optimal number of blocks increased as the correlation coefficient, the number of treatments, and the number of subjects per treatment increased.

TO BLOCK OR COVARY A CONCOMITANT VARIABLE: WHICH IS MORE POWERFUL?

INTRODUCTION

Educational experiments often involve assigning students to treatments. Traditional one-way analysis of variance can be used to analyze the differences among treatments. However, differences among students, such as, gender, socio-economic status, and level of ability, often mask or obscure the effects of a treatment (Kennedy & Bush, 1985; Kirk, 1982). Nuisance variation due to such differences can be extracted from the error variance. By controlling the concomitant (nuisance) variable, researchers often reduce the background noise, increase the precision, and enhance the statistical power of a design (Bonett, 1982; Keppel, 1991; Maxwell & Delaney, 1984). The most widely used procedures to harness the power of a concomitant variable are block designs and the analysis of covariance.

Statement of the Problem

Evidence suggests that approximately 70% of published research in the behavioral sciences uses analysis of variance techniques (Glass & Hopkins, 1984). Many of these studies are analyses of either block or covariance designs. Whether to block or covary and how many blocks to be used if a block design is chosen become important questions and the answers may differ according to the experimental conditions. The purpose of this study is to determine which procedure should be used given a set of experimental conditions.

Significance of the Study

The decisions on whether to block or covary and how many blocks to be used if a block design is selected are often based on rules of thumb with little empirical support. An empirical study that can offer the scientific foundation on which to base such decisions is desirable. The results of this study should provide guidelines to help researchers decide the appropriate procedures to be used.

REVIEW OF THE RELATED LITERATURE

Historical Review of the Problem

By employing a concomitant variable, researchers can control the nuisance variance, reduce the error, increase the precision, and enhance the efficiency of an experimental design (Cochran & Cox, 1950; Cochran & Cox, 1957; Federer & Schlottfeldt, 1954; Fisher, 1937; Fisher, 1973a; Fisher, 1973b; Kennedy and Bush 1985; Keppel, 1973; Keppel, 1991; Kirk, 1982; Lindquist, 1953; Maxwell & Delaney, 1990; Myers, 1979). In the two classic books, The Design of Experiments and Statistical Methods for Research Workers, Fisher (1937; 1973a) developed the analysis of variance of block designs and the analysis of covariance. He demonstrated that the precision of an experimental design could be improved by controlling a concomitant variable using the two analysis techniques.

Lindquist (1953) used the term, treatments-by-levels design, which consists of more than one subject in a cell, to differentiate it from the randomized complete block design, which consists of only one subject in a cell. The treatments-by-levels design is also called the treatments-by-blocks design (Kennedy & Bush 1985). Lindquist recommended that the treatments-by-blocks design be used in preference to the analysis of covariance because (1) the treatments-by-blocks design required much less restrictive assumptions than the analysis of covariance, (2) the computational procedures were considerably simpler with the treatments-by-blocks design, and (3) the use of treatments-by-blocks design permitted a study on the simple effects of the treatments at any given block.

Gourlay (1953) compared the analysis of covariance with the randomized complete block design in which blocks were formed by matching subjects on the concomitant variable. He recommended that the analysis of covariance be used in preference to the matching block technique; this view was shared by Greenberg (1953) in a similar study.

Federer (1955) favored the block design over the analysis of covariance. He offered the following rule of thumb: "If the experimental variation cannot be controlled by stratification (blocking), then measure related variates and use covariance" (p. 483-484). However, he also pointed out that "it may be more advantageous to use covariance than to use stratification, since fewer degrees of freedom are usually required to control the variation" (p. 484).

Cox (1957) developed the Apparent Imprecision measure and used it to compare the analysis of covariance with the randomized complete block design in which blocks were formed by ranking subjects on the concomitant variable. He found that the randomized complete block design was somewhat better than the analysis of covariance if the correlation coefficient was less than .6 while the analysis of covariance became appreciably better than the randomized complete block design when the correlation coefficient was .8 or more. He suggested that the analysis of covariance was preferable to the randomized complete block design only if the correlation coefficient between the concomitant and the dependent variable was at least .6.

The most rigorous research on this topic was conducted by Feldt (1958). He used Cox's Apparent Imprecision measure to compare three experimental designs. The three experimental designs being compared were (1) stratification (blocking), (2) the analysis of covariance, and (3) the analysis of variance of difference scores. Feldt found the analysis of variance of difference scores was the least precise procedure; for ρ [correlation] < .4 block designs results in approximately equal or greater precision than the analysis of covariance; for $\rho \geq .6$ the advantage is in favor of the analysis of covariance; and for $\rho <$ .2 and small values of the number of subjects neither the analysis of covariance nor block designs yields appreciably greater precision than the one-way analysis of variance. Feldt also provided a table for the optimal number of blocks to be used if block designs were selected. He concluded that the optimal number of blocks tended to be larger for (1) larger values of correlation coefficients, (2) larger numbers of subjects, and (3) smaller numbers of treatments. This study should be considered the classic study comparing block designs with the analysis of covariance; its findings have been most often quoted by textbooks in the area of experimental designs (e.g., Cook & Campbell, 1979; Dayton, 1970; Kennedy and Bush, 1985; Keppel, 1991; Kirk, 1982; Myers, 1979). However, Feldt's concept of optimal blocking was not supported by Chuang's (1978) study, which found that block designs would become more powerful as the number of blocks increased and "the power of BLOCKING even at its maximum was slightly smaller than that of COVARIANCE" (p. 37).

In a block design, subjects are usually grouped into blocks before the experiment according to the value of the concomitant variable. However, there are times that the value of the concomitant variable is not available before the experiment. When blocks are formed after the experiment, the block design is defined as a post-hoc block design. Keppel (1973) gave the following advantages of post-noc block designs over the analysis of covariance: (1) reduction in computational effort, (2) free from the stricter assumptions of the analysis of covariance, and (3) possibility of testing the treatments-by-blocks interaction. However, he also pointed out two disadvantages of post-hoc blocking: (1) the inability to calculate the within-groups mean square when cells have fewer than 2 subjects, and (2) the inability to adjust the treatment means for differences on the concomitant variable.

Post-hoc blocking is popular because the value of the concomitant variable can be unknown before the experiment. Nevertheless, Myers (1979) pointed out the danger of abusing post-hoc block designs by demonstrating that reordering scores within each treatment does not change the treatment means but generally reduces the error variance, resulting in significant Fs which "merely reflect the reduction in error variance due to blocking rather than any variability due to treatments" (p. 155). However, he did not consider the loss of degrees of freedom with the block design.

Bonett (1982) compared post-hoc block designs with the analysis of covariance and offered the following rule:

> *If the assumptions for each method can be satisfied and if the probability of a Type II error is of concern, the analysis of covariance will be preferred when the form of the regression equation is known but the magnitude of the correlation is known. Post-hoc blocking, on the other hand, will be preferred when the magnitude of the correlation is known. (p. 38)*

A study employing the Monte Carlo method and using statistical power as the criterion variable to compare block designs and the analysis of covariance was performed by Maxwell and Delaney (1984). Their study was limited to two treatments. The procedures they compared were based on the following two dimensions: (1) the method of assignment and (2) the method of data analysis. Each of the two dimensions had three levels: the concomitant variable was (1) ignored, (2) categorized, and (3) continuous. This resulted in nine procedures being compared. Maxwell and Delaney (1984) favored the analysis of covariance over block designs. They argued that

> *the recommendation of most experimental design texts to consider the correlation between the dependent and concomitant variables in choosing the best technique for utilizing a concomitant variable is incorrect. Instead, the two factors that should be considered are whether scores on the concomitant variable are available for all subjects prior to assigning any subjects to treatment conditions and whether the relationship of the dependent and concomitant variables is linear. (p. 136)*

They also illustrated that the Apparent Imprecision measure, which was used in the Cox (1957) and Feldt (1958) studies, might provide a different perspective from statistical power, but, the Apparent Imprecision measure and statistical power are not independent.

Summary

While some research favored block designs, others preferred the analysis of covariance. Based on the historical review of the problem, it is summarized that "the relative merits of blocking and ANCOVA are more complicated, because neither is uniformly superior to the other" (Maxwell & Delaney, 1984, p. 136). It is likely that different procedures may be preferable to the others depending on the sets of experimental conditions. One consequence of applying block designs and the analysis of covariance, which has been often neglected in early research but frequently stressed in recent research, is the decrease of the probability of the Type II error, i.e., the increase in statistical power.

Based on the review of the related literature, it is suggested that future research should examine the problem based on three dimensions: (1) how subjects are assigned, (2) how data are analyzed, and (3) the distributions of and the relationship between the concomitant and the dependent variables (i.e., considering the assumptions of block designs and the analysis of covariance). The experimental conditions should include three factors: (1) the number of treatments, (2) the number of subjects per treatment, and (3) the magnitude of the relationship between the concomitant and the dependent variables. The criterion variables on which to base the comparison should be the statistical power, the Type I error (α), and the Apparent Imprecision measure.

Justification of the Study

This section provides the rationale for selecting statistical power as the criterion variable and using computer generated data to simulate the experiment.

Statistical Power as the Criterion Variable

The expressions; "reduce error", "increase precision", "enhance efficiency", and "maximize statistical power;" have frequently been used interchangeably to describe the objective of employing a concomitant variable in block designs and the analysis of variance (e.g., Bonett, 1982; Kennedy & Bush, 1985; Maxwell & Delaney, 1984). Among these expressions, the term "statistical power" is the most unambiguously understood one and is operationally defined in most statistical texts.

The neglect of statistical power in research, textbooks, and curricula has been brought to the attention of the research community. As Cohen (1962; 1977; 1988; 1992) has stressed, one of the most pervasive threats to the validity of the statistical conclusions reached by behavioral research is low statistical power. The investigation of statistical power in experiment designs has gained more and more popularity (Chase & Tucker, 1976; Sedlmeier & Gigerenzer, 1989).

Computer Simulation

This is an empirical study using the Monte Carlo method to simulate the experiment. The Monte Carlo method has been used effectively in examining many properties of statistics (Harwell, Rubinstein, Hayes, & Olds, 1992; Shapiro, Wilk, & Chen, 1968; Wilcox, Charlin, & Thompson, 1986). Computer simulations have many advantages. "We can often simulate situations more readily on the computer than perform the corresponding experiments in real life"; "one can also easily vary parameters in computer experiments"; and "furthermore, the simulations tend to be very flexible in that a whole multitude of differing models can be simulated with relative ease with essentially the same computer code" (Jain, 1992, p. 2). Therefore, using a high speed computer to calculate statistical power based on empirical sampling is the most direct and effective way to answer the research questions of this study.

PROCEDURES

This study compared five analysis procedures under 48 sets of experimental conditions using empirical power as the criterion (dependent) variable. The five analysis procedures were the one-way analysis of variance; two-block, four-block, and eight-block block designs; and the analysis of covariance. The 48 sets of experimental conditions were the combinations of four levels of the number of treatments (T; 2, 3, 4, 5),

three levels of the number of subjects per treatment (N; 8, 40, 72), and four levels of the correlation coefficient between the concomitant and the dependent variable (C; .00, .28, .56, .84). For each experimental condition, 1,000 sets of data were generated by the computer. Each set of data was analyzed by all five analysis procedures with the significance level (α) set at .05. The proportion of significant analyses was the empirical power; for example, if 600 out of the 1,000 analyses were significant, the empirical power would be .6. Each resulting empirical power was entered as one observation in each cell. The procedure to generate and analyze the 1,000 sets of data was repeated two more times for each of the 48 experimental conditions. This resulted in a repeated measure four-way factorial design with three observations (i.e,. three empirical powers) in each of the 240 (5 X 48) cells.

Calculation of the Effect Size

Statistical power is determined by three major factors: (1) the significance level, (2) the sample size, and (3) the effect size (Dayton, Schafer, & Rogers 1973; Hinkle, Wiersma, & Jurs, 1988; Lipsey, 1990; Sawyer & Ball, 1981). Statistical power increases as the significance level, the sample size, and the effect size increases. In order to make the comparison of the five analysis procedures more meaningful, the one-way analysis of variance was treated as the control group by setting its power at .50. This also would allow the powers of the other procedures to increase or decrease as a function of the conditions. Therefore, the effect sizes which would achieve a .50 power for the one-way analysis of variance under given experimental conditions needed to be calculated before the experiment. Calculation of effect sizes was based on tables and formulae in Cohen's (1988) book. The effect size index is defined as

$$f = \frac{\sigma_m}{\sigma},$$

where σ is the population standard deviation and

$$\sigma_m = \sqrt{\sum_{i=1}^{k} \frac{(m_i - m)^2}{k}},$$

where m_i is the population mean of the ith treatment, m is the grand population mean, and k is the number of treatments. The effect size index was calculated by the following formula:

$$f=\sqrt{\frac{n_{.05}}{400(n-1)}},$$

where n is the number of subjects per treatment, and $n_{.05}$ is the required number of subjects per treatment to achieve a desired power when f is equal to .05. The value of $n_{.05}$ can be obtained from Cohen's tables (p. 381-389). For example, for three treatments with α = .05, the table shows that $n_{.05}$ is 662. If the number of subjects per treatment is 8, then

$$f=\sqrt{\frac{662}{400(8-1)}}=0.4862392.$$

When the means are equally spaced, the distance between the largest and the smallest mean can be calculated by the following formula:

$$d=2f\sqrt{\frac{3(k-1)}{k+1}}.$$

In this case,

$$d=2(0.4862392)\sqrt{\frac{3(3-1)}{3+1}}=1.1910379.$$

In order to equally space the means, this number should be divided by two:

$$\frac{1.1910379}{2}=0.5955189$$

Thus, to achieve a .5 statistical power for one-way analysis of variance when α =.05, n = 8, and k = 3, the population mean of the first treatment should be set at m_1, the second at m_1 + 0.5955, and the third at m_1 + 1.1910. Since the results of the analyses would be the same for any value of m_1, the population mean of the first treatment was always set at 0 in this experiment. The following is the table of the calculated effect sizes to achieve a statistical power of .50 for the one-way analyses of variance under given experimental conditions.

Table 1

Calculated Effect Size

	Effect sizes for N=8	Effect sizes for N=40	Effect sizes for N=72
T=2	1.0481	0.4440	0.3291
T=3	0.5955, 1.1910	0.2523, 0.5046	0.1870, 0.3740
T=4	0.4060, 0.8121, 1.2181	0.1720, 0.3440, 0.5161	0.1275, 0.2550, 0.3825
T=5	0.3030, 0.6059, 0.9089, 1.2118	0.1284, 0.2567, 0.3851, 0.5134	0.0951, 0.1903, 0.2854, 0.3805

Generation and Analyses of the Data

The generation and the analyses of the data were accomplished using a computer simulation system running on an IBM 3090/400E mainframe computer. Bivariate correlated data were generated using the SAS commands provided by Clark and Woodward (1992). These commands generated random data from a bivariate (the concomitant and the dependent variables) normal distribution with means of 0, variances of 1, and a user-specified correlation coefficient. Random samples were generated separately for each treatment. Only the means of the dependent variable were transformed based on the calculated effect sizes in Table 1, while the other parameters remained unchanged. Data in each treatment were grouped into 2, 4, and 8 blocks by their ranks on the concomitant variable. For example, to group 40 subjects into 4 blocks, the top 10 ranked subjects would be in the first block, the 11-20 ranked subjects would be in the second block, the 21-30 ranked subjects would be in the third block, and the 31-40 ranked subjects would be in the fourth block.

The computer simulation system included one executable file and two SAS programs (International Business Machines, 1988a; International Business Machines, 1988b; SAS Institute Inc., 1990a; SAS Institute Inc., 1990b). For each of the 48 experimental conditions, the executable file ran the first SAS program 1,000 times, then ran the second SAS program. The computer code for the condition of the number of treatments (T) equal to 5, the number of subjects per treatment (N) equal to 72, and the correlation coefficient (C) equal to .84 are listed in Appendix A. The first SAS program generated a set of data, analyzed that set of data with the five analysis procedures being compared, and output the results of the analyses to a data file. After the first SAS program ran 1,000 times, the data file contained 1,000 records of the results of the analyses.

The second SAS program calculated the empirical power based on the 1,000 records. In order to obtain three observations per cell, the executable file ran three times for each of the 48 experimental conditions. Totally, there were 144,000 (1,000 X 3 X 48) sets of data generated and 720,000 (5 X 144,000) analyses conducted.

A seed must be provided to generate random data using SAS random functions. The values of seeds can be any integer ranging from 1 to 2^{31} - 2 (i.e., 2,147,483,646). Users can let the computer clock set an initial seed by specifying a value of 0. Using the computer clock to generate random data was tested and found to have three problems: (1) The computer clock may not increment enough to generate different data; (2) The computer clock may generate repeated or patterned data; and (3) the program may not be executed because the computer clock generates invalid seeds. Therefore, positive seeds were used instead of the computer clock. Using positive seeds also makes the experiment replicable. In order to systematically and representatively employ the seeds, the minimum seed value, 1, was used as the first initial seed; it was incremented by 2,147,483 for each run of the first SAS program, and by 14,913 for each run of the executable file. The two incremented values were obtained by dividing the maximum seed value by 1,000 and 144,000. Thus, all seeds were equally spaced in the range between the minimum and maximum seed values. The initial seeds used for each executable file are listed in Appendix B.

Hypotheses

This study tested the following null hypotheses using empirical power as the dependent variable. If a null hypothesis was rejected, its follow-ups were conducted, and the null hypotheses following it was ignored.

HO_1: There will be no significant differences at the .05 level for the four-way interaction.
HO_2: There will be no significant differences at the .05 level for the three-way interaction.
HO_3: There will be no significant differences at the .05 level for the two-way interaction.
HO_4: There will be no significant differences at the .05 level for the main effect.

RESULTS

The resulting power values are listed in Appendix C. The raw data were analyzed to test the null hypotheses. The results of the analyses are summarized in Table 2.

Table 2

ANOVA Summary

Tests of hypotheses using S(T*N*C) as the error term						Tests of hypotheses using P*S(T*N*C) as the error term					
Source	DF	SS	Mean Square	F Value	Pr > F	Source	DF	SS	Mean Square	F Value	Pr > F
T	3	0.167672	0.05589083	116.31	0.0001	P	4	2.226688	0.55667191	12117.48	0.0001
N	2	0.050754	0.02537705	52.81	0.0001	T*P	12	0.073263	0.00610521	132.90	0.0001
C	3	8.853233	2.95107773	6141.08	0.0001	N*P	8	0.026325	0.00329068	71.63	0.0001
T*N	6	0.010605	0.00176756	3.68	0.0025	C*P	12	2.820703	0.23505859	5116.69	0.0001
T*C	9	0.103833	0.01153697	24.01	0.0001	T*N*P	24	0.005493	0.00022889	4.98	0.0001
N*C	6	0.002530	0.00042162	0.88	0.5146	T*C*P	36	0.051654	0.00143483	31.23	0.0001
T*N*C	18	0.004716	0.00026198	0.55	0.9285	N*C*P	24	0.004126	0.00017191	3.74	0.0001
S(T*N*C)	96	0.046133	0.00048055			T*N*C*P	72	0.002742	0.00003808	0.83	0.8336
						P*S(T*N*C)	384	0.017641	0.00004594		
						Total	719	14.468110			

Note. T: Number of Treatments, N: Number of Subjects per Treatment, C: Correlation Coefficient, P: Procedure, S: Data Set.

The four-way interaction (T*N*C*P) was not significant while the three-way interactions of T*N*P, T*C*P, and N*C*P were significant. The cell means for the significant three-way interactions are listed in Tables 3, 4, and 5 respectively. Simple simple effects were tested at the significance level of .01. Non-significant simple simple effects are indicated by "NS" in the last cells of the corresponding rows and columns. Tukey's Honest Significant Difference (HSD) values are provided in the table notes for multiple comparisons. The cell means for the other combinations are listed in Appendix D.

Table 3

Means for the Interaction of the Number of Treatments, the Number of Subjects per Treatment, and the Procedure (T*N*P)

		ANOVA	TWO-BLOCK	FOUR-BLOCK	EIGHT-BLOCK	ANCOVA
T2	N08	.498	.561	.580	.546	.628
	N40	.500	.575	.604	.613	.663
	N72	.506	.577	.606	.615	.663
T3	N08	.497	.583	.606	.599	.646
	N40	.501	.593	.627	.639	.666
	N72	.502	.593	.628	.639	.666
T4	N08	.501	.601	.633	.630	.658
	N40	.502	.609	.645	.657	.668
	N72	.501	.613	.650	.663	.674
T5	N08	.503	.615	.649	.648	.666
	N40	.504	.626	.663	.677	.674
	N72	.498 NS	.620	.656	.668	.671

Note. P@T*N = .008, T*N@P =.015, NS: Non-significant.

Table 4

Means for the Interaction of the Number of Treatments, the Correlation Coefficient, and the Procedure (T*C*P)

		ANOVA	TWO-BLOCK	FOUR-BLOCK	EIGHT-BLOCK	ANCOVA
T2	C00	.504	.502	.498	.481	.490
	C28	.502	.518	.523	.511	.523
	C56	.496	.576	.605	.601	.645
	C84	.503	.689	.759	.774	.947
T3	C00	.505	.505	.501	.495	.496
	C28	.497	.519	.525	.522	.528
	C56	.502	.601	.636	.643	.653
	C84	.496	.735	.821	.844	.960
T4	C00	.503	.503	.504	.496	.498 NS
	C28	.496	.527	.535	.534	.526
	C56	.498	.623	.663	.676	.677
	C84	.508	.778	.868	.894	.967
T5	C00	.505	.503	.504	.499	.497 NS
	C28	.501	.534	.544	.545	.526
	C56	.507	.640	.686	.699	.683
	C84	.494 NS	.804	.890	.916	.975

Note. HSD: P@T*C=.009, T*C@P=.019, NS: Non-significant.

Table 5

Means for the Interaction of the Number of Subjects per Treatment, the Correlation Coefficient, and the Procedure (N*C*P)

		ANOVA	TWO-BLOCK	FOUR-BLOCK	EIGHT-BLOCK	ANCOVA
N08	C00	.502	.499	.494	.469	.478
	C28	.500	.521	.525	.506	.512
	C56	.498	.602	.634	.624	.653
	C84	.499	.739	.816	.824	.955
N40	C00	.505	.505	.505	.505	.503 NS
	C28	.502	.527	.537	.541	.534
	C56	.502	.614	.652	.667	.670
	C84	.498	.759	.844	.874	.964
N72	C00	.505	.506	.505	.504	.505 NS
	C28	.495	.525	.534	.537	.530
	C56	.502	.615	.657	.673	.670
	C84	.503 NS	.758	.844	.872	.968

Note. HSD: P@N*C=.008, N*C@P=.015, NS: Non-significant

Comparison of Analysis Procedures

When the correlation coefficient between the concomitant and dependent variables was equal to .00, the one-way ANOVA was as powerful as or more powerful than blocking and covariance. The difference was significant when the number of treatments and the number of subjects per treatment were small. As the correlation coefficient increased, the power of employing a concomitant variable became more and more significant—from 50 to over 90 percent. With the correlation coefficient equal to .28, the optimal blocking procedure was as powerful as or slightly more powerful than covariance. Covariance was favored when the correlation was moderate or high. However, with moderate correlation, blocking could be as powerful as or slightly more powerful than covariance when the number of treatments and the number of subjects per treatment were large.

Comparison of Conditions

The ranks of the power values of the 48 sets of conditions for all and each of the analysis procedures are listed in Appendix E. The pattern of the ranks showed that the three blocking procedures and the ANCOVA became more powerful as (1) the correlation coefficient, (2) the number of treatments, and (3) the number of subjects per treatment increased. Among the three factors, the correlation coefficient was dominant. The ranks for the one-way ANOVA showed a random pattern with all power values being approximately .50, which provided evidence that the power of the one-way ANOVA had been successfully controlled.

Optimal Number of Blocks

The results did not provide the specific optimal number of blocks to be used under each experimental condition because the experiment did not include all possible numbers of blocks. The results did indicate that the optimal number of blocks increased as the correlation coefficient, the number of subjects per treatment, and the number of treatments increased.

SUMMARY

Different procedures should be used depending on the set of experimental conditions. The correlation coefficient between the concomitant and the dependent variable was the critical factor that should influence

the choice. The one-way ANOVA was the best choice when the correlation was zero, block designs were preferred when the correlation was low, and the analysis of covariance achieved the highest power when the correlation was high. With moderate correlation, block designs should be selected only when the number of treatments and the number of subjects per treatment were large; otherwise, the analysis of covariance should be used. Block designs and the analysis of covariance became more powerful and the optimal number of blocks for a block design increased as the correlation coefficient, the number of treatments, and the number of subjects per treatment increased.

Discussion

The levels of the experimental conditions were chosen to be equally spaced and to be representative of real world situations. The four levels of the number of treatments represented the most commonly used numbers of treatments; the three levels of the number of subjects per treatment represented small, medium, and large sample sizes; and the four levels of the correlation coefficient represented zero, low, moderate, and high correlations. The results of the study provided a guide to help researchers decide the appropriate procedures to be used under different experimental conditions.

This study had the following characteristics:

1. It controlled the power of the one-way ANOVA to prevent ceiling and floor effects; this also made the comparisons more meaningful as the one-way ANOVA was treated as the control group.
2. Unlike most of the Monte Carlo studies that had only one observation in each cell and provided only descriptive statistics, this study had multiple observations in each cell and provided inferential in addition to descriptive results.
3. Basing the sample size on the number of subjects per treatment instead of the total number of subjects made the interpretation of the results more meaningful.
4. The computer simulation system consisted only of SAS programs, which were much shorter and more understandable than equivalent programs written in Fortran or other programming languages.

One limitation of this study was that it did not include all possible blocking procedures. Since the results showed that the optimal number of blocks increased as the sample size increased, block designs could

become more powerful if other blocking procedures with more blocks were used. The primary disadvantage of using SAS rather than a programming language for Monte Carlo simulation is that SAS uses more computer CPU time. However, this disadvantage can be overcome by using a high speed computer such as the IBM 3090/400E computer used in this study.

Recommendations for Future Research

Several pilot studies which examined the parameters and distributions of the mean,variance, and correlation coefficient were conducted before the experiment, and the resulting sampling distributions of the statistics were checked after this experiment. The inspection found that the computer simulation system generated data that met predetermined specification. Furthermore, before the experiment, the power of the one-way ANOVA was controlled at .50. The resulting empirical powers of the one-way ANOVA had a mean of .50 and a variance of .0001; also the mean squares for S(T*N*C) and P*S(T*N*C) were .00048055 and .00004594 respectively and the pooled mean square error was .00013286—all supporting the precision of the data generation procedures. Since the computer simulation system is able to generate accurate data and examine the problems effectively, it is recommended that future research adapt the system to examine related problems.

This study does not include the treatments-by-blocks interaction in the block designs since the interaction does not exist in the population. Future studies can examine the effects of including the interaction using essentially the same computer codes, or, by varying the parameters of the population, examine the effects of including and excluding the interaction when the interaction exists in the population. The optimal number of blocks for a block design could be investigated by including other feasible blocking schemes such as 5-, 10-, 20-, and 40-block block designs for the condition of 40 subjects per treatment.

The greatest contribution of this study may not be the specific results reported here, but the potential for examining many other situations. The computer simulation system developed for this study could be modified easily for a multitude of other studies. For example, it could be used to investigate other criteria such as Type I errors, examine other levels of the experimental conditions, or test other blocking methods in addition to the post-hoc blocking used in this study.

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Appendix A

COMPUTER CODES

Executable File

```
/*     */
ADDRESS COMMAND
"ERASE PVALUE DATA A"
NUMERIC DIGITS 10
TIME = 1
DO WHILE TIME < 1001
SEED = 2132560 + (TIME -1) * 2147483
"EXECIO 1 DISKW" NEWSEED DATA A
"(STRING" SEED
"EXEC SAS T57284"
"ERASE NEWSEED DATA A"
TIME=TIME+1
END
"EXEC SAS T57284P"
```

First SAS Program (T57284 SAS A)

```
CMS FILEDEF INDATA DISK NEWSEED DATA
A;
CMS FILEDEF PVALUE DISK PVALUE DATA A
(LRECL 210 BLKSIZE 210 RECFM FBS;
CMS FILEDEF SASLIST DISK T57284
LISTING A;
DATA BIVNORM (DROP=I);
   INFILE INDATA;
      INPUT SEED;
   DO I=1 TO 72;
      GROUP=1;
      X=RANNOR(SEED);

Y=.84*X+SQRT(1-.84**2)*RANNOR(SEED);
      OUTPUT;
   END;
   DO I=1 TO 72;
      GROUP=2;
      X=RANNOR(SEED);

Y=.84*X+SQRT(1-.84**2)*RANNOR(SEED);
      Y=0.0951+Y;
      OUTPUT;
   END;
   DO I=1 TO 72;
      GROUP=3;
      X=RANNOR(SEED);

Y=.84*X+SQRT(1-.84**2)*RANNOR(SEED);
      Y=0.1903+Y;
      OUTPUT;
   END;
   DO I=1 TO 72;
      GROUP=4;
      X=RANNOR(SEED);

Y=.84*X+SQRT(1-.84**2)*RANNOR(SEED);
      Y=0.2854+Y;
      OUTPUT;
   END;
   DO I=1 TO 72;
      GROUP=5;
      X=RANNOR(SEED);

Y=.84*X+SQRT(1-.84**2)*RANNOR(SEED);
      Y=0.3805+Y;
      OUTPUT;
   END;
PROC SORT;
   BY GROUP X;
DATA BIVNORM;
   SET BIVNORM;
   BN=MOD(_N_,72); IF BN=0 THEN
   BN=72;
   IF BN<=36 THEN B2=1; ELSE B2=2;
   IF BN<=18 THEN B4=1; ELSE IF
   BN<=36 THEN B4=2;
   ELSE IF BN<=54 THEN B4=3; ELSE
   B4=4;
   IF BN<=9 THEN B8=1; ELSE IF BN<=18
   THEN B8=2;
```

```
   ELSE IF BN<=27 THEN B8=3; ELSE IF
   BN<=36 THEN B8=4;
   ELSE IF BN<=45 THEN B8=5; ELSE IF
   BN<=54 THEN B8=6;
   ELSE IF BN<=63 THEN B8=7; ELSE
B8=8;
PROC PRINT;
PROC CORR DATA=BIVNORM;
   VAR X Y;
   BY GROUP;
PROC GLM;
   CLASS GROUP;
   MODEL Y=GROUP/SS3;
PROC GLM;
   CLASS GROUP B2;
   MODEL Y=GROUP B2/SS3;
PROC GLM;
   CLASS GROUP B4;
   MODEL Y=GROUP B4/SS3;
PROC GLM;
   CLASS GROUP B8;
   MODEL Y=GROUP B8/SS3;
PROC GLM;
   CLASS GROUP;
   MODEL Y=GROUP X/SS3;
DATA;
   INFILE SASLIST;
   INPUT WORD1 $ WORD2 $ @;
   FILE PVALUE MOD;
   IF WORD1 = 'X' AND WORD2 ='72'
   THEN DO;
      INPUT MEAN STDDEV;
      PUT MEAN 6.4 STDDEV 6.4 @;
      INPUT Y $ N MEAN STDDEV;
      PUT MEAN 6.4 STDDEV 6.4 @;
      END;
   ELSE  IF WORD1="X" AND WORD2 =
      '1.00000' THEN DO;
      INPUT CORR;
      PUT CORR 6.4 @;
      END;
   ELSE  IF WORD1="GROUP" AND WORD2 =
     '4' THEN DO;
      INPUT SS MS F PR;
      PUT PR 6.4 @;
      INPUT BLOCK $ DF SS MS F PR;
      PUT PR 6.4 @;
      END;
```

Second SAS Program (T57284P SAS A)

```
CMS FILEDEF INDATA DISK PVALUE DATA
A;
DATA PVALUE;
INFILE INDATA;
INPUT (G1XMEAN G1XSD G1YMEAN G1YSD
       G1CORR
       G2XMEAN G2XSD G2YMEAN G2YSD
       G2CORR
       G3XMEAN G3XSD G3YMEAN G3YSD
       G3CORR
       G4XMEAN G4XSD G4YMEAN G4YSD
       G4CORR
       G5XMEAN G5XSD G5YMEAN G5YSD
       G5CORR
       GROUP1B BLOCK1B GROUP2B
       BLOCK2B GROUP4B BLOCK4B
       GROUP8B   BLOCK8B   GROUPANC
BLOCKANC) (35* 6.4);
     TOTAL=0;
     G1BSG=0;
     B1BSG=0;
     G2BSG=0;
     B2BSG=0;
     G4BSG=0;
     B4BSG=0;
     G8BSG=0;
     B8BSG=0;
     GANCSG=0;
     BANCSG=0;
     TOTAL=1;
     IF GROUP1B <= 0.05 THEN G1BSG=1;
     IF BLOCK1B <= 0.05 THEN B1BSG=1;
     IF GROUP2B <= 0.05 THEN G2BSG=1;
     IF BLOCK2B <= 0.05 THEN B2BSG=1;
     IF GROUP4B <= 0.05 THEN G4BSG=1;
     IF BLOCK4B <= 0.05 THEN B4BSG=1;
     IF GROUP8B <= 0.05 THEN G8BSG=1;
     IF BLOCK8B <= 0.05 THEN B8BSG=1;
     IF  GROUPANC  <=  0.05  THEN
GANCSG=1;
     IF  BLOCKANC  <=  0.05  THEN
BANCSG=1;
PROC FREQ;
     TABLE G1BSG -- BANCSG;
PROC SUMMARY DATA=PVALUE;
     VAR G1XMEAN -- BANCSG;
     OUTPUT OUT = DESCRIPT;
PROC PRINT DATA=DESCRIPT;
PROC  UNIVARIATE  DATA=PVALUE  PLOT
NORMAL;
     VAR G1XMEAN -- BLOCKANC;
```

Appendix B

INITIAL SEEDS

SEED	CONDITION*	SEED	CONDITION*	SEED	CONDITION*
1	T2N08C00DS1	715825	T2N08C00DS2	1431649	T2N08C00DS3
14914	T2N08C28DS4	730738	T2N08C28DS5	1446562	T2N08C28DS6
29827	T2N08C56DS7	745651	T2N08C56DS8	1461475	T2N08C56DS9
44740	T2N08C84DS10	760564	T2N08C84DS11	1476388	T2N08C84DS12
59653	T2N40C00DS13	775477	T2N40C00DS14	1491301	T2N40C00DS15
74566	T2N40C28DS16	790390	T2N40C28DS17	1506214	T2N40C28DS18
89479	T2N40C56DS19	805303	T2N40C56DS20	1521127	T2N40C56DS21
104392	T2N40C84DS22	820216	T2N40C84DS23	1536040	T2N40C84DS24
119305	T2N72C00DS25	835129	T2N72C00DS26	1550953	T2N72C00DS27
134218	T2N72C28DS28	850042	T2N72C28DS29	1565866	T2N72C28DS30
149131	T2N72C56DS31	864955	T2N72C56DS32	1580779	T2N72C56DS33
164044	T2N72C84DS34	879868	T2N72C84DS35	1595692	T2N72C84DS36
178957	T3N08C00DS37	894781	T3N08C00DS38	1610605	T3N08C00DS39
193870	T3N08C28DS40	909694	T3N08C28DS41	1625518	T3N08C28DS42
208783	T3N08C56DS43	924607	T3N08C56DS44	1640431	T3N08C56DS45
223696	T3N08C84DS46	939520	T3N08C84DS47	1655344	T3N08C84DS48
238609	T3N40C00DS49	954433	T3N40C00DS50	1670257	T3N40C00DS51
253522	T3N40C28DS52	969346	T3N40C28DS53	1685170	T3N40C28DS54
268435	T3N40C56DS55	984259	T3N40C56DS56	1700083	T3N40C56DS57
283348	T3N40C84DS58	999172	T3N40C84DS59	1714996	T3N40C84DS60
298261	T3N72C00DS61	1014085	T3N72C00DS62	1729909	T3N72C00DS63
313174	T3N72C28DS64	1028998	T3N72C28DS65	1744822	T3N72C28DS66
328087	T3N72C56DS67	1043911	T3N72C56DS68	1759735	T3N72C56DS69
343000	T3N72C84DS70	1058824	T3N72C84DS71	1774648	T3N72C84DS72
357913	T4N08C00DS73	1073737	T4N08C00DS74	1789561	T4N08C00DS75
372826	T4N08C28DS76	1088650	T4N08C28DS77	1804474	T4N08C28DS78
387739	T4N08C56DS79	1103563	T4N08C56DS80	1819387	T4N08C56DS81
402652	T4N08C84DS82	1118476	T4N08C84DS83	1834300	T4N08C84DS84
417565	T4N40C00DS85	1133389	T4N40C00DS86	1849213	T4N40C00DS87
432478	T4N40C28DS88	1148302	T4N40C28DS89	1864126	T4N40C28DS90
447391	T4N40C56DS91	1163215	T4N40C56DS92	1879039	T4N40C56DS93
462304	T4N40C84DS94	1178128	T4N40C84DS95	1893952	T4N40C84DS96
477217	T4N72C00DS97	1193041	T4N72C00DS98	1908865	T4N72C00DS99
492130	T4N72C28DS100	1207954	T4N72C28DS101	1923778	T4N72C28DS102
507043	T4N72C56DS103	1222867	T4N72C56DS104	1938691	T4N72C56DS105
521956	T4N72C84DS106	1237780	T4N72C84DS107	1953604	T4N72C84DS108
536869	T5N08C00DS109	1252693	T5N08C00DS110	1968517	T5N08C00DS111
551782	T5N08C28DS112	1267606	T5N08C28DS113	1983430	T5N08C28DS114
566695	T5N08C56DS115	1282519	T5N08C56DS116	1998343	T5N08C56DS117
581608	T5N08C84DS118	1297432	T5N08C84DS119	2013256	T5N08C84DS120
596521	T5N40C00DS121	1312345	T5N40C00DS122	2028169	T5N40C00DS123
611434	T5N40C28DS124	1327258	T5N40C28DS125	2043082	T5N40C28DS126
626347	T5N40C56DS127	1342171	T5N40C56DS128	2057995	T5N40C56DS129
641260	T5N40C84DS130	1357084	T5N40C84DS131	2072908	T5N40C84DS132
656173	T5N72C00DS133	1371997	T5N72C00DS134	2087821	T5N72C00DS135
671086	T5N72C28DS136	1386910	T5N72C28DS137	2102734	T5N72C28DS138
685999	T5N72C56DS139	1401823	T5N72C56DS140	2117647	T5N72C56DS141
700912	T5N72C84DS142	1416736	T5N72C84DS143	2132560	T5N72C84DS144

* T: the number of treatments, N: the number of subjects per treatment, C: the correlation coefficient, DS: the data set.

Appendix C

LAYOUT OF THE DESIGN AND RAW DATA

For Two Treatments (T2)								For Three Treatment (T3)					
N	C	S	ANO	2B	4B	8B	COV	S	ANO	2B	4B	8B	COV
N08	C00	DS1	.489	.483	.465	.436	.449	DS37	.525	.521	.510	.498	.504
		DS2	.505	.495	.484	.428	.449	DS38	.489	.490	.466	.450	.465
		DS3	.496	.495	.493	.427	.473	DS39	.511	.513	.509	.496	.486
	C28	DS4	.505	.505	.513	.469	.511	DS40	.492	.511	.517	.497	.517
		DS5	.521	.525	.533	.480	.502	DS41	.499	.519	.526	.517	.517
		DS6	.488	.500	.486	.462	.489	DS42	.487	.512	.505	.488	.503
	C56	DS7	.497	.583	.602	.569	.632	DS43	.499	.582	.605	.592	.634
		DS8	.499	.562	.591	.557	.629	DS44	.501	.589	.617	.609	.633
		DS9	.473	.551	.574	.537	.607	DS45	.495	.603	.630	.620	.636
	C84	DS10	.511	.679	.751	.741	.935	DS46	.497	.720	.805	.820	.946
		DS11	.485	.659	.710	.700	.941	DS47	.483	.715	.787	.802	.955
		DS12	.507	.701	.758	.750	.925	DS48	.490	.724	.798	.804	.954
N40	C00	DS13	.500	.499	.495	.498	.503	DS49	.517	.515	.515	.511	.513
		DS14	.508	.510	.512	.506	.513	DS50	.499	.494	.495	.496	.491
		DS15	.500	.500	.503	.502	.503	DS51	.494	.493	.493	.492	.491
	C28	DS16	.502	.522	.530	.533	.536	DS52	.499	.512	.524	.522	.529
		DS17	.497	.514	.519	.529	.532	DS53	.509	.530	.537	.545	.535
		DS18	.503	.528	.537	.534	.532	DS54	.517	.535	.538	.543	.557
	C56	DS19	.488	.573	.602	.611	.651	DS55	.500	.586	.622	.635	.646
		DS20	.514	.588	.617	.628	.664	DS56	.512	.609	.648	.662	.687
		DS21	.496	.588	.621	.628	.655	DS57	.488	.610	.654	.672	.668
	C84	DS22	.514	.700	.775	.795	.954	DS58	.487	.747	.833	.879	.958
		DS23	.487	.686	.753	.790	.950	DS59	.510	.744	.829	.852	.957
		DS24	.493	.694	.780	.806	.959	DS60	.482	.742	.831	.862	.965
N72	C00	DS25	.524	.522	.521	.520	.521	DS61	.513	.511	.513	.514	.513
		DS26	.496	.495	.496	.496	.491	DS62	.500	.505	.503	.501	.503
		DS27	.518	.517	.515	.512	.510	DS63	.500	.500	.502	.498	.501
	C28	DS28	.499	.520	.524	.527	.548	DS64	.491	.512	.521	.523	.531
		DS29	.494	.511	.526	.523	.525	DS65	.483	.513	.525	.531	.530
		DS30	.510	.534	.538	.543	.532	DS66	.497	.523	.529	.534	.529
	C56	DS31	.501	.573	.610	.622	.655	DS67	.522	.630	.665	.680	.656
		DS32	.482	.563	.601	.612	.652	DS68	.504	.611	.645	.665	.656
		DS33	.513	.602	.630	.647	.657	DS69	.498	.590	.635	.651	.663
	C84	DS34	.520	.711	.781	.806	.952	DS70	.492	.737	.822	.847	.968
		DS35	.491	.682	.757	.785	.962	DS71	.516	.758	.840	.863	.968
		DS36	.519	.690	.768	.792	.948	DS72	.504	.731	.840	.864	.968

Note. T: the number of treatments, N: the number of subjects per treatment,
C: the correlation coefficient, DS: the data set.

For Four Treatments (T4)								For Five Treatments (T5)					
N	C	S	ANO	2B	4B	8B	COV	S	ANO	2B	4B	8B	COV
N08	C00	DS73	.502	.501	.501	.476	.489	DS109	.510	.500	.491	.478	.476
		DS74	.493	.492	.487	.477	.486	DS110	.506	.507	.507	.500	.493
		DS75	.497	.494	.502	.479	.481	DS111	.507	.502	.508	.487	.490
	C28	DS76	.481	.515	.516	.510	.525	DS112	.496	.525	.536	.531	.513
		DS77	.510	.538	.547	.527	.527	DS113	.522	.548	.554	.543	.528
		DS78	.513	.534	.534	.530	.502	DS114	.488	.522	.530	.519	.513
	C56	DS79	.503	.626	.655	.661	.659	DS115	.514	.629	.667	.680	.693
		DS80	.505	.619	.662	.671	.663	DS116	.506	.639	.682	.680	.696
		DS81	.476	.603	.637	.638	.676	DS117	.509	.632	.685	.679	.679
	C84	DS82	.519	.768	.853	.864	.964	DS118	.487	.796	.877	.892	.974
		DS83	.519	.758	.850	.865	.968	DS119	.499	.788	.872	.886	.968
		DS84	.492	.765	.855	.867	.961	DS120	.496	.789	.875	.900	.969
N40	C00	DS85	.498	.499	.502	.498	.492	DS121	.507	.508	.509	.511	.503
		DS86	.501	.501	.499	.496	.493	DS122	.516	.515	.518	.520	.513
		DS87	.517	.520	.518	.518	.516	DS123	.503	.504	.507	.509	.502
	C28	DS88	.495	.521	.541	.545	.544	DS124	.502	.543	.553	.559	.539
		DS89	.497	.530	.538	.542	.530	DS125	.509	.537	.548	.556	.523
		DS90	.490	.519	.530	.533	.523	DS126	.500	.531	.547	.551	.534
	C56	DS91	.509	.620	.650	.668	.670	DS127	.523	.649	.690	.713	.689
		DS92	.503	.625	.669	.688	.673	DS128	.501	.646	.699	.717	.690
		DS93	.493	.631	.667	.682	.674	DS129	.501	.639	.680	.700	.676
	C84	DS94	.521	.805	.892	.917	.974	DS130	.485	.815	.910	.935	.974
		DS95	.497	.769	.856	.891	.963	DS131	.482	.797	.883	.917	.969
		DS96	.497	.772	.878	.907	.967	DS132	.519	.831	.911	.941	.975
N72	C00	DS97	.506	.508	.507	.510	.512	DS133	.491	.490	.488	.487	.498
		DS98	.514	.517	.516	.514	.514	DS134	.497	.497	.498	.494	.495
		DS99	.498	.499	.500	.500	.498	DS135	.505	.506	.506	.503	.507
	C28	DS100	.511	.543	.550	.551	.542	DS136	.478	.517	.533	.534	.513
		DS101	.489	.527	.533	.536	.529	DS137	.501	.535	.543	.546	.531
		DS102	.478	.518	.529	.530	.510	DS138	.515	.544	.556	.562	.539
	C56	DS103	.510	.638	.692	.702	.700	DS139	.494	.621	.663	.688	.669
		DS104	.501	.626	.666	.684	.696	DS140	.500	.658	.705	.718	.684
		DS105	.484	.615	.673	.694	.680	DS141	.518	.647	.701	.713	.672
	C84	DS106	.494	.792	.871	.911	.960	DS142	.494	.805	.894	.919	.982
		DS107	.521	.794	.387	.919	.974	DS143	.487	.808	.900	.926	.980
		DS108	.508	.779	.374	.907	.972	DS144	.496	.807	.891	.925	.982

Note. T: the number of treatments, N: the number of subjects per treatment,
C: the correlation coefficient, DS: the data set.

Appendix D

Mean Tables

For Two Treatments (T2)							For Three Treatments (T3)				
N	C	ANO	2B	4B	8B	COV	ANO	2B	4B	8B	COV
N08	C00	.497	.491	.481	.430	.457	.508	.508	.495	.481	.485
	C28	.505	.510	.511	.470	.501	.493	.514	.516	.501	.512
	C56	.490	.565	.589	.554	.623	.498	.591	.617	.607	.634
	C84	.501	.680	.740	.730	.934	.490	.720	.797	.809	.952
N40	C00	.503	.503	.503	.502	.506	.503	.501	.501	.500	.498
	C28	.501	.521	.529	.532	.533	.508	.526	.533	.537	.540
	C56	.499	.583	.613	.622	.657	.500	.602	.641	.656	.667
	C84	.498	.693	.769	.797	.954	.493	.744	.831	.864	.960
N72	C00	.513	.511	.511	.509	.507	.504	.505	.506	.504	.506
	C28	.501	.522	.529	.531	.535	.490	.516	.525	.529	.530
	C56	.499	.579	.614	.627	.655	.508	.610	.648	.665	.658
	C84	.510	.694	.769	.794	.954	.504	.742	.834	.858	.968
For Four Treatments (T4)							For Five Treatments (T5)				
N08	C00	.497	.496	.497	.477	.485	.508	.503	.502	.488	.486
	C28	.501	.529	.532	.522	.518	.502	.532	.540	.533	.518
	C56	.495	.616	.651	.657	.666	.510	.633	.678	.680	.689
	C84	.510	.764	.853	.865	.964	.494	.791	.875	.893	.970
N40	C00	.505	.507	.506	.504	.500	.509	.509	.511	.513	.506
	C28	.494	.523	.536	.540	.532	.504	.537	.549	.555	.532
	C56	.502	.625	.662	.679	.672	.508	.645	.690	.710	.685
	C84	.505	.782	.875	.905	.968	.495	.814	.901	.931	.973
N72	C00	.506	.508	.508	.508	.508	.498	.498	.497	.495	.500
	C28	.493	.529	.537	.539	.527	.498	.532	.544	.547	.528
	C56	.498	.626	.677	.693	.692	.504	.642	.690	.70	.675
	C84	.508	.788	.877	.912	.969	.492	.807	.895	.923	.981

Note. T: the number of treatments, N: the number of subjects per treatment,
C: the correlation coefficient.

		C00	C28	C56	C84
T2	N08	0.497	0.499	0.564	0.717
	N40	0.503	0.523	0.595	0.742
	N72	0.510	0.524	0.595	0.744
T3	N08	0.496	0.507	0.590	0.753
	N40	0.501	0.529	0.613	0.779
	N72	0.505	0.518	0.618	0.781
T4	N08	0.490	0.521	0.617	0.791
	N40	0.505	0.525	0.628	0.807
	N72	0.508	0.525	0.637	0.811
T5	N08	0.497	0.525	0.638	0.805
	N40	0.510	0.535	0.648	0.823
	N72	0.497	0.530	0.643	0.820

	N08	N40	N72
T2	0.563	0.591	0.593
T3	0.586	0.605	0.606
T4	0.605	0.616	0.620
T5	0.616	0.629	0.623

	C00	C28	C56	C84
T2	0.495	0.515	0.585	0.735
T3	0.500	0.518	0.607	0.771
T4	0.501	0.524	0.627	0.803
T5	0.502	0.530	0.643	0.816

	ANOVA	2BLOCK	4BLOCK	8BLOCK	ANCOV
T2	0.501	0.571	0.596	0.592	0.651
T3	0.500	0.590	0.620	0.626	0.659
T4	0.501	0.608	0.643	0.650	0.667
T5	0.502	0.620	0.656	0.665	0.670

	C00	C28	C56	C84
N08	0.489	0.513	0.602	0.766
N40	0.505	0.528	0.621	0.788
N72	0.505	0.524	0.623	0.789

	ANOVA	2BLOCK	4BLOCK	8BLOCK	ANCOV
N08	0.500	0.590	0.617	0.606	0.650
N40	0.502	0.601	0.635	0.647	0.668
N72	0.502	0.601	0.635	0.646	0.668

	ANOVA	2BLOCK	4BLOCK	8BLOCK	ANCOV
C00	0.504	0.503	0.501	0.493	0.495
C28	0.499	0.524	0.532	0.528	0.526
C56	0.501	0.610	0.648	0.655	0.664
C84	0.500	0.752	0.835	0.857	0.962

T2	T3	T4	T5
0.582	0.599	0.614	0.623

N08	N40	N72
0.593	0.610	0.610

C00	C28	C56	C84
0.499	0.522	0.616	0.781

ANOVA	2BLOCK	4BLOCK	8BLOCK	ANCOV
0.501	0.597	0.629	0.633	0.662

Note. T: the number of treatments, N: the number of subjects per treatment,
C: the correlation coefficient.

Appendix E

RANKS OF THE FORTY-EIGHT EXPERIMENTAL CONDITIONS

All Procedures		One-way ANOVA		Two-block		Four-block		Eight-block		ANCOVA	
Condition*	Power	Condition*	Power	Condition*	Power	Condition*	Power	Condition*	Power	Condition*	Power
T2N08C00	0.471	T2N08C56	0.490	T2N08C00	0.491	T2N08C00	0.481	T2N08C00	0.430	T2N08C00	0.457
T4N08C00	0.490	T3N08C84	0.490	T4N08C00	0.496	T3N08C00	0.495	T2N08C28	0.470	T3N08C00	0.485
T3N08C00	0.496	T3N72C28	0.490	T5N72C00	0.498	T4N08C00	0.497	T4N08C00	0.477	T4N08C00	0.485
T5N08C00	0.497	T5N72C84	0.492	T3N40C00	0.501	T5N72C00	0.497	T3N08C00	0.481	T5N08C00	0.486
T5N72C00	0.497	T3N08C28	0.493	T2N40C00	0.503	T3N40C00	0.501	T5N08C00	0.488	T3N40C00	0.498
T2N08C28	0.499	T3N40C84	0.493	T5N08C00	0.503	T5N08C00	0.502	T5N72C00	0.495	T4N40C00	0.500
T3N40C00	0.501	T4N72C28	0.493	T3N72C00	0.505	T2N40C00	0.503	T3N40C00	0.500	T5N72C00	0.500
T2N40C00	0.503	T4N40C28	0.494	T4N40C00	0.507	T3N72C00	0.506	T3N08C28	0.501	T2N08C28	0.501
T3N72C00	0.505	T5N08C84	0.494	T3N08C00	0.508	T4N40C00	0.506	T2N40C00	0.502	T2N40C00	0.506
T4N40C00	0.505	T4N08C56	0.495	T4N72C00	0.508	T4N72C00	0.508	T3N72C00	0.504	T3N72C00	0.506
T3N08C28	0.507	T5N40C84	0.495	T5N40C00	0.509	T2N08C28	0.511	T4N40C00	0.504	T5N40C00	0.506
T4N72C00	0.508	T2N08C00	0.497	T2N08C28	0.510	T2N72C00	0.511	T4N72C00	0.508	T2N72C00	0.507
T2N72C00	0.510	T4N08C00	0.497	T2N72C00	0.511	T5N40C00	0.511	T2N72C00	0.509	T4N72C00	0.508
T5N40C00	0.510	T2N40C84	0.498	T3N08C28	0.514	T3N08C28	0.516	T5N40C00	0.513	T3N08C28	0.512
T3N72C28	0.518	T3N08C56	0.498	T3N72C28	0.516	T3N72C28	0.525	T4N08C28	0.522	T4N08C28	0.518
T4N08C28	0.521	T4N72C56	0.498	T2N40C28	0.521	T2N40C28	0.529	T3N72C28	0.529	T5N08C28	0.518
T2N40C28	0.523	T5N72C00	0.498	T2N72C28	0.522	T2N72C28	0.529	T2N72C28	0.531	T4N72C28	0.527
T2N72C28	0.524	T5N72C28	0.498	T4N40C28	0.523	T4N08C28	0.532	T2N40C28	0.532	T5N72C28	0.528
T4N40C28	0.525	T2N40C56	0.499	T3N40C28	0.526	T3N40C28	0.535	T5N08C28	0.533	T3N72C28	0.530
T4N72C28	0.525	T2N72C56	0.499	T4N08C28	0.529	T4N40C28	0.536	T3N40C28	0.537	T4N40C28	0.532
T5N08C28	0.525	T3N40C56	0.500	T4N72C28	0.529	T4N72C28	0.537	T4N72C28	0.539	T5N40C28	0.532
T3N40C28	0.529	T2N08C84	0.501	T5N08C28	0.532	T5N08C28	0.540	T4N40C28	0.540	T2N40C28	0.533
T5N72C28	0.530	T2N40C28	0.501	T5N72C28	0.532	T5N72C28	0.544	T5N72C28	0.547	T2N72C28	0.535
T5N40C28	0.535	T2N72C28	0.501	T5N40C28	0.537	T5N40C28	0.549	T2N08C56	0.554	T3N40C28	0.540
T2N08C56	0.564	T4N08C28	0.501	T2N08C56	0.565	T2N08C56	0.589	T5N40C28	0.555	T2N08C56	0.623
T3N08C56	0.590	T4N40C56	0.502	T2N72C56	0.579	T2N40C56	0.613	T3N08C56	0.607	T3N08C56	0.634
T2N40C56	0.595	T5N08C28	0.502	T2N40C56	0.583	T2N72C56	0.614	T2N40C56	0.622	T2N72C56	0.655
T2N72C56	0.595	T2N40C00	0.503	T3N08C56	0.591	T3N08C56	0.617	T2N72C56	0.627	T2N40C56	0.657
T3N40C56	0.613	T3N40C00	0.503	T3N40C56	0.602	T3N40C56	0.641	T3N40C56	0.656	T3N72C56	0.658
T4N08C56	0.617	T5N72C00	0.504	T3N72C56	0.610	T3N72C56	0.648	T4N08C56	0.657	T4N08C56	0.666
T3N72C56	0.618	T3N72C84	0.504	T4N08C56	0.616	T4N08C56	0.651	T3N72C56	0.665	T3N40C56	0.667
T4N40C56	0.628	T5N40C28	0.504	T4N40C56	0.625	T4N40C56	0.662	T4N40C56	0.679	T4N40C56	0.672
T4N72C56	0.637	T5N72C56	0.504	T4N72C56	0.626	T4N72C56	0.677	T5N08C56	0.680	T5N72C56	0.675
T5N08C56	0.638	T2N08C28	0.505	T5N08C56	0.633	T5N08C56	0.678	T4N72C56	0.693	T5N40C56	0.685
T5N72C56	0.643	T4N40C00	0.505	T5N72C56	0.642	T5N40C56	0.690	T5N72C56	0.706	T5N08C56	0.689
T5N40C56	0.648	T4N40C84	0.505	T5N40C56	0.645	T5N72C56	0.690	T5N40C56	0.710	T4N72C56	0.692
T2N08C84	0.717	T4N72C00	0.506	T2N08C84	0.680	T2N08C84	0.740	T2N08C84	0.730	T2N08C84	0.934
T2N40C84	0.742	T3N08C00	0.508	T2N40C84	0.693	T2N40C84	0.769	T2N72C84	0.794	T3N08C84	0.952
T2N72C84	0.744	T3N40C28	0.508	T2N72C84	0.694	T2N72C84	0.769	T2N40C84	0.797	T2N40C84	0.954
T3N08C84	0.753	T3N72C56	0.508	T3N08C84	0.720	T3N08C84	0.797	T3N08C84	0.809	T2N72C84	0.954
T3N40C84	0.779	T4N72C84	0.508	T3N72C84	0.742	T3N40C84	0.831	T3N72C84	0.858	T3N40C84	0.960
T3N72C84	0.781	T5N08C00	0.508	T3N40C84	0.744	T3N72C84	0.834	T3N40C84	0.864	T4N08C84	0.964
T4N08C84	0.791	T5N40C56	0.508	T4N08C84	0.764	T4N08C84	0.853	T4N08C84	0.865	T3N72C84	0.968
T5N08C84	0.805	T5N40C00	0.509	T4N40C84	0.782	T4N40C84	0.875	T5N08C84	0.893	T4N40C84	0.968
T4N40C84	0.807	T2N72C84	0.510	T4N72C84	0.788	T5N08C84	0.875	T4N40C84	0.905	T4N72C84	0.969
T4N72C84	0.811	T4N08C84	0.510	T5N08C84	0.791	T4N72C84	0.877	T4N72C84	0.912	T5N08C84	0.970
T5N72C84	0.820	T5N08C56	0.510	T5N72C84	0.807	T5N72C84	0.895	T5N72C84	0.923	T5N40C84	0.973
T5N40C84	0.823	T2N72C00	0.513	T5N40C84	0.814	T5N40C84	0.901	T5N40C84	0.931	T5N72C84	0.981

* T: the number of treatments, N: the number of subjects per treatment, C: the correlation coefficient.